Economic Insecurity and Demand for Immigration Enforcement

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Abstract

Does increased labor market scarcity lead to more local immigration enforcement? We answer this question by evaluating the impact of the Great Recession on the likelihood that local law enforcement agencies partner with Immigration and Customs Enforcement through 287(g) agreements. Using a difference-in-differences design, we find that commuting zones facing higher unemployment rates during the Great Recession were significantly more likely to adopt stricter immigration enforcement policies. The results are concentrated among commuting zones with above median share in pre-Recession immigrant population, workforce in the construction industry, and foreign born representation in routine manual occupations. Our results are consistent with the hypothesis that adverse labor market shocks lead to greater support of policies restricting immigrants ability to compete in local labor markets.

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I Introduction

Economic distress often influences policy preferences (Mayda, 2006; Facchini and Mayda, 2009; Autor et al., 2020), triggering political pressure to reshape policies (Feigenbaum and Hall, 2015; Che et al., 2022). Understanding whether economic shocks impact the adoption of immigration policy is particularly important, as immigration consistently ranks among the most salient political issues in the U.S. (Feigenbaum et al., 2025) and has substantial welfare implications for both the citizen and immigrant populations (Borjas, 2001; Card, 2001). Furthermore, public understanding of the welfare effects of immigration often diverges from empirical evidence, creating conditions under which economic shocks may have a strong influence on policy adoption.

The divergence between perceptions and reality becomes especially consequential during economic contractions, when distributional concerns about labor market competition are most salient. Prior work shows that immigrants play a stabilizing role throughout the U.S. economy. Specifically, immigrants are responsive to regional economic opportunities (Borjas, 2001), absorb a disproportionate share of unemployment fluctuations (Orrenius and Zavodny, 2010), and demonstrate greater geographic mobility than natives during economic downturns (Cadena and Kovak, 2016).[1] However, there still exist widespread misperceptions about both immigrants' economic impacts and characteristics (Alesina et al., 2018, 2023; Stantcheva, 2024). A consequence of this asymmetric information and distributional uncertainty is that voters may support policies that contradict both aggregate and individual welfare (Ali et al., 2025).

Whether past economic downturns contributed to the expansion of immigration enforcement policies adopted since the turn of the millennium is therefore an important question, given that these policies impose large direct costs on local jurisdictions (Nguyen and Gill, 2010) and have been shown to worsen labor market outcomes for both immigrant and native workers (Lee et al., 2022; East et al., 2023; García and Gutiérrez-Li, 2023).[2] However, empirically examining how economic shocks influence immigration policy adoption is challenging, as these policies are largely determined at the federal level, and only exceptionally large shocks are likely to shift policy preferences enough to affect adoption. Our study seeks to overcome these issues by leveraging the adoption of a local immigration policy and geographic variation in changes in the unemployment rate as a result of the Great Recession. Our context centers around 287(g) agreements, which are partnerships between local law enforcement agencies and U.S. Immigration and Customs Enforcement (ICE) established through memorandums of agreements (MOA).[3] These agreements were first adopted in Florida in

[1] Gálvez-Iniesta (2024) reinforces this evidence, showing that immigration helped mitigate the impact of the Great Recession on native unemployment in Spain.

[2] Secure Communities, 287(g) programs and E-Verify were all created following the turn of the century.

[3] Specifically, 287(g) agreements authorize local law enforcement agencies to perform federal immigration enforce-

2002 and had expanded to over 150 counties across the US by 2020.[4] Importantly, these programs represent a discrete policy choice by elected officials, making them a measurable outcome through which to test whether economic shocks trigger the adoption of enforcement policies.

We begin with the universe of MOAs between each local law enforcement agency and ICE between 2002 and 2020.[5] We use these data to construct a commuting zone-level measure of policy adoption based on whether any local law enforcement agency within the commuting zone (CZ) had an active 287(g) agreement in a given year. We then identify CZs facing greater exposure to the Great Recession by leveraging geographic variation in the change in unemployment rate between 2007 and 2009 from Yagan (2019). The resulting dataset tracks the adoption of 287(g) agreements across CZs facing higher or lower changes in the unemployment rate between 2007 and 2009, both before and after the Great Recession occurred.

Using these datasets, we estimate the causal effects of the Great Recession on the likelihood of 287(g) adoption using a standard difference-in-differences model. Specifically, we compare the change in the likelihood of any local law enforcement agency within a CZ having an active 287(g) agreement in the years before and after the Great Recession for CZs facing higher versus lower changes in the unemployment rate between 2007 and 2009. We find that, on average, a 1 percentage point (p.p.) increase in a commuting zone's unemployment rate (due to the Great Recession) was associated with a 1.3 p.p. (around 25%) increase in the likelihood of any local law enforcement agency within the CZ adopting 287(g). We find larger effects when comparing CZs with 287(g) applicants to those without any applicants. The expanded 287(g) "applicant" criteria include CZs with agencies that were approved by ICE, and thus considered participants, as well as CZs with agencies whose applications were denied. The stronger results from this analysis suggest that economic distress from the Great Recession increased local demand for immigration enforcement even in places where 287(g) partnerships were never actualized.

We further explore the impacts of the Great Recession on the likelihood of having an active 287(g) agreement by disaggregating the results based on several baseline characteristics. The baseline population demographics of a CZ may impact the the likelihood of adopting a 287(g) agreement if the perceived number of immigrants in a community plays a role in determining policy preferences. Baseline crime rates may influence the adoption of 287(g) programs since the policy is marketed towards improving safety in local neighborhoods. The Great Recession hit the construction and routine manual occupations the hardest, so we may expect CZs that were initially reliant on these

ment functions and are advertised a method to combat crime in local communities.

[4]Notably, ICE denied participation to many law enforcement agencies that applied for the 287(g) program.

[5]While the first 287(g) agreement was adopted in 2002, our analysis focuses on the adoption of this program at local law enforcement agencies (county and city policy departments) as discussed in Section III.

industries to have the strongest responses. Conversely, areas reliant on the agriculture industry may be less open to these programs since the workforce is comprised largely of foreign-born workers.

While we find no evidence of heterogeneous results across baseline incarceration rates, there are significant differences in outcomes based on population demographics and industry composition. We find that CZs hit harder by the Great Recession and initially high population shares of Hispanic individuals, immigrants, individuals working in construction, or areas with more over-representation of foreign-born individuals in routine manual jobs saw the largest increase in the likelihood of having an active 287(g) program. These results are consistent with the idea that additional local immigration enforcement is enacted in response to increased perceived competition for scarce jobs following an economic downturn, suggesting that anti-immigrant sentiment could be rooted in labor market competition (Cotofan et al., 2024; Giuliano and Spilimbergo, 2025).

Given our results, we pay particular attention to the possibility that differential trends in population demographics and economic characteristics could generate our findings. In particular, eventually harder-hit CZs may have seen changes in either population inflows or the (proxy) immigrant share, both of which could independently influence the likelihood of adopting a 287(g) agreement. We also want to rule out that there were differential trends in economic characteristics prior to the Great Recession. To address this concern, we employ several event-study specifications to examine the differences in these characteristics prior to the year 2007. We find that CZs more severely affected by the Great Recession exhibited no significant differences in the population growth rate, the Hispanic share of the population, the birth rate to foreign-norn mothers, the employment growth rate, the unemployment rate or the employment-to-population ratio relative to less-affected areas. We take these results as strong evidence that the increase in the likelihood of adopting a 287(g) agreement is not due to differential trends across CZs.

Our results are robust to model specification choices and the possibility of concurrent changes beyond the Great Recession explaining the increase in the adoption of 287(g) programs. First, we show our results are robust to relaxing the strong parallel trends assumption established in our continuous difference-in-differences context by using the alternative estimation strategy proposed in Callaway et al. (2024). Second, we show that are results are similar when we alter the definition of commuting zones, or measure the severity of the Great Recession using a Bartik shock as in Hershbein and Kahn (2018). Lastly, we argue that the severity of the Great Recession is unrelated to other concurrent changes that may impact the likelihood of adopting a 287(g) program. Specifically, we show that commuting zones facing larger changes in the unemployment rate between 2007 and 2009 saw no differential changes in political outcomes or crime rates that could explain our findings.

A remaining concern is that 287(g) partnerships reflected their stated public-safety purpose rather

than responses to local economic distress. To assess this possibility, we examine administrative data on Notices to Appear (NTAs) to understand how 287(g) programs were implemented. NTAs are formal charging documents that initiate immigration removal proceedings and can be categorized as either criminal or non-criminal based on the underlying charges. Our analysis shows that following 287(g) activation, there was a sharp increase in non-criminal NTAs while criminal NTAs remained unchanged, suggesting that 287(g) programs were deployed to broaden immigration enforcement in ways potentially unrelated to public safety. These results, together with the absence of increases in crime rates following the Great Recession, provides direct evidence that economic distress drove demand for 287(g) rather than through correlated factors like crime.

Our paper contributes to the growing economics literature on the determinants of current immigration policy. Specifically, to the best of our knowledge, we provide the first causal evidence that economic shocks, independent of immigration flows, drive the adoption of immigration policy, suggesting that recessions activate demand for enforcement.

While much of the existing literature in this domain has focused on the relationship between anti-immigration sentiment and broader social factors, such as individuals' contact with immigrants (Bursztyn et al., 2021; Steinmayr, 2021) and the misconceptions about immigrants' characteristics (Alesina et al., 2023), there has been inconsistent evidence reflecting a fundamental gap in how the question has been studied.[6] Research has focused predominantly on individual attitudes through surveys and voting behaviors rather than policy outcomes themselves. As a result, we know relatively more about what shapes immigration preferences than whether and how these preferences translate into actual policy changes. Furthermore, whether economic or cultural explanations drive policy preference as a central focus obscures deeper insights. A broader interpretation of the literature suggests that these mechanisms are not mutually exclusive, but rather that economic shocks activate both material and cultural responses, with labor market stress providing the catalyst for anti-immigrant mobilization (Choi et al., 2024; Autor et al., 2020; Alsan et al., 2020; Edo et al., 2019; Halla et al., 2017).

Standard economic theory predicts that increased competition in the labor market influences policy preferences. Research consistent with this line of reasoning reveals that economic factors can have a substantial impact on sentiments towards immigrants when labor market competition is present, even if such competition has low prevalence in the general population (Mayda, 2006; Malhotra et al., 2013; Cotofan et al., 2024). However, empirical evidence has provided mixed results. Several studies show that the connection between economic factors and anti-immigrant sentiment

[6] Alesina and Tabellini (2024) find that while economic factors can drive anti-immigrant policy preferences, cultural factors consistently play a considerable role.

is weak (Citrin et al., 1997; Card et al., 2012; Hainmueller and Hopkins, 2014). The key insight in our findings is that recessions make labor market competition concerns both more prevalent and more intense, which triggers a policy response. The distinction is that economic conditions likely have nonlinear effects where small changes in labor market conditions produce weak attitudinal responses, but large contractions during recessions cross a threshold that translates latent concerns into political action.

Second, we provide the first systematic evidence on the determinants of local immigration enforcement partnerships. While previous research has estimated the effects of 287(g) and similar programs on economic outcomes, including migration patterns (Watson, 2013), self-employment (García and Gutiérrez-Li, 2023), and labor market outcomes (East et al., 2023), no prior work has examined why localities adopted these policies. This gap is considerable. If policy adoption reflects underlying economic or political conditions that also affect labor markets, then estimated effects of enforcement may conflate the direct impact of the policy with selection into the treatment group. Our results show that adoption is driven by economic distress rather than crime rates or immigrant inflows, which has implications for interpreting the broader enforcement literature and for understanding which jurisdictions are most likely to adopt similar policies.

The remainder of this paper is organized as follows. In Section II, we provide background information on the 287(g) program. Section III summarizes the data used in this paper and describes our constructed measures of 287(g) and Great Recession severity. Section IV describes the reduced-form empirical strategy and lays out the regression specifications. Section V contains the main results, which include our heterogeneity analysis. Section VI describes our validity checks, while Section VII discusses alternative mechanisms beyond labor market conditions for 287(g) adoption including a discussion the impacts of the Great Recession on political outcomes and crime. Section VIII offers conclusions from this research.

II Background on 287(g) Programs

The 287(g) program is named after section 287(g) of the Immigration and Nationality Act, that allows the Department of Homeland Security (DHS) to enter into formal written agreements with state or local law enforcement agencies. The program deputizes state and local law enforcement officers to perform certain functions of federal immigration agents (American Immigration Council, 2021). In general, deputized officers are authorized to interview individuals to ascertain their im-

migration status; check DHS databases for information on individuals; issue immigration detainers to hold individuals until ICE takes custody; issue a Notice to Appear; make recommendations for detention and immigration bond; and transfer noncitizens into ICE custody.[7]

Figure 1 plots the adoption of 287(g) agreements over time. Figure 1 shows that very few 287(g) agreements were implemented before 2007, with a sharp increase following the Great Recession. The number of active 287(g) agreements remained relatively stable between 2008 and 2012 before witnessing a sharp decline following charges of racial profiling in Alamance County, North Carolina and Maricopa County Arizona. Law enforcement partnerships with ICE received further negative attention following Arizona v. United States where the supreme court struck down several aspects of an Arizona law, which among other provisions made it a crime for immigrants to not carry proof of legal residence (Arizona Stat. § SB1070). Popularity for 287(g) quickly reignited at the start of the Trump administration with 2020 being the year with the highest number of active agreements.

It is worth noting how local law enforcement agencies enter into the program. Previously, agencies would express interest in forming a partnership and ICE would evaluate the proposal based on the available resources at the law enforcement agency and the capability of the agency to serve as a force multiplier for ICE's interior immigration enforcement. If satisfactory, ICE would then sign a memorandum of agreement. Agreements were much more likely to be signed during the Trump administration as it was believe that ICE focused on a strategy of approving as many agencies as possible without regard to any benefits (American Immigration Council, 2021). We will discuss the implications of this change in the next section.

Opponents of 287(g) policies argue that these agreements are used to incite racial profiling of the local Hispanic population (American Civil Liberties Union, 2022) and can also serve as a measure of local anti-immigration sentiment (Capps et al., 2011). Furthermore, 287(g) agreements are voluntarily entered into by local law enforcement agencies – most often county sheriff's departments – through the signing of a Memorandum of Agreement with ICE. Since practically all county sheriff's departments are ran by elected officials, an agency signing a 287(g) agreement can be directly influenced by local politics. This design feature of 287(g) policies provides a more direct route from local voter preferences to policy when compared to other measures of anti-immigrant sentiment which is often centered around local vote shares of parties with anti-immigrant views (Tabellini, 2020).

[7] Originally, there were four types of 287(g) agreements local law enforcement agencies could adopt. However, since 2012 only the jail enforcement and warrant service officer models are allowed. We do not separate our results by 287(g) model, but instead focus on the likelihood of adopting any agreement.

III Data

The data for this project come from two primary sources: U.S. Immigration and Customs Enforcement and publicly available data on the Great Recession from Yagan (2019). The archives of U.S. Immigration and Customs Enforcement website keeps the memorandums of agreement (MOA) for each 287(g) agreement ever implemented across the United States (Immigration and Customs Enforcement, 2025). The information contained within these MOAs include the law enforcement agency (LEA) participating in the program, the date of adoption, the program type, and the termination date. LEA's wishing to continue participation in 287(g) past the termination date must extend the MOA as discussed below. We collect data for all agreements signed between 2002 and 2020.

Sheriff's departments make up the largest share of 287(g) agreements in our data (around 80% during our sample period) and the average agreement is in place for 8 years. Table 1 shows a complete set of summary statistics for the set of 287(g) agreements we consider in each year of our sample period. Importantly, we do not witness any local law enforcement agencies (sheriff's departments, city/county police departments, or county jail/corrections departments) signing a 287(g) agreement until 2005; therefore, we limit the sample time frame to the years 2005 through 2020 in our main specification. We focus on the adoption of 287(g) agreements from local law enforcement agencies (LLEA) because the connection between a local labor market shock and the adoption of these policies is looser at the state-level.

The other primary source of data comes from Yagan (2019). Yagan (2019) measures the intensity of the Great Recession (from here on called the Great Recession shock) as the percentage point change in a CZ's unemployment rate from 2007 to 2009. CZs are collections of adjacent counties that approximate local labor markets (Tolbert and Sizer, 1996; Autor and Dorn, 2013). Autor and Dorn (2013) identify 722 commuting zones in the continental United States (excluding Alaska and Hawaii) that we use as our main sample of interest. The annual CZ unemployment rates are computed by aggregating monthly population-weighted county-level unemployment rates from the monthly BLS LAUS series to the CZ-month level, then averaging evenly within CZ-years across months. In our main specification, CZs are defined based on their 1990 delineation, but we discuss robustness to this definition in Section VI.

Figure 2a plots the geographic variation in the adoption of 287(g) agreements from local law enforcement agencies across commuting zones from 2005 to 2020.[8] The adoption of these programs is concentrated along the East Coast, the Southwest and Eastern Texas. Commuting zones (CZ) can be recorded as adopting more than one 287(g) agreement during our sample time frame because either there are multiple local law enforcement agencies within a commuting zone that adopt one of these

[8] An equivalent map for 287(g) applications between 2005 and 2011 is shown in Appendix Figure A1.

programs (e.g., two separate county sheriff's offices within the same CZ begin a partnership with ICE), or a local law enforcement agency within the CZ entered into a 287(g) agreement, terminated the agreement and then entered into a new agreement during our sample time frame.[9] In our main specification, we are only interested in whether any local law enforcement agency within a CZ has an active 287(g) agreement; therefore, we ignore cases of multiple active agreements within a year and set our outcome variable of interest to zero in years where the only 287(g) agreement within a CZ was no longer active.

Figure 2b displays the geographic variation in the severity of the Great Recession shock as in Yagan (2019). Commuting zones within the Mountain West, South Atlantic and Midwest witnessed the largest change in the unemployment rate from 2007 to 2009. By comparing Figures 2a and 2b, we can begin to see a positive association between the severity of the Great Recession and the likelihood of adopting a 287(g) agreement during our sample time frame. While not a formal difference-in-difference design, Figure 2 provides a visual preview of our findings.

We supplement our primary data with information from three additional sources: the 2000 Decennial Census, information from Autor et al. (2021), and data made available from Pedroza (2019). Information from the 2000 Decennial Census and Autor et al. (2021) provides data on baseline commuting zone characteristics including information on demographics (total population and population by race/age/nativity) and economic conditions (median household income, labor force statistics, and exposure to other labor market shocks). We use these data in our heterogeneity analysis and to construct Table 2, which reports summary statistics for commuting zones that faced an above or below median Great Recession shock. Columns (1)-(3) present the sample means and differences across these two groups of commuting zones for the year 2000.

Commuting zones that faced an above median Great Recession shock are different from those that faced a smaller change in the unemployment rate from 2007 to 2009. Commuting zones hit harder by the Great Recession were larger, had a lower average median household income, had lower share of the population receiving a Bachelor's degree, had a higher share of the population working in the construction industry, lower shares of the population working in the agriculture industry, and had greater exposure to other labor market shocks (namely, increased import competition from China and increased automation of routine jobs). Importantly, our empirical strategy does not rely on the equality of the pre-policy summary statistics. Instead, identification requires that the change in outcomes for the CZs hit less hard by the Great Recession are what those hit hardest would have

[9]For example, Bay County Sheriff's Department first adopted a 287(g) program in 2008 that was terminated in 2012 and a new agreement was signed in 2019. Many of the the 287(g) agreement terminations occurred in 2012 following a Supreme Court ruling outlawing the task force model.

experienced had the Great Recession not occurred. We discuss this assumption and the inclusion of baseline controls in further detail in later sections.

Data made available from Pedroza (2019) provides information on all local law enforcement agencies that applied for a 287(g) program but were not granted an agreement between 2005 and 2011. These data include information on the local law enforcement agency applying for the program, the year the application was submitted and the outcome of the application. Appendix Table A1 presents summary statistics on the set of unsuccessful 287(g) applications in each year. These data are advantageous for two reasons. First, combining denied applications with active agreements provides the full set of local law enforcement agencies interested in participating in the program. Second, if one is concerned about our outcome variable of interest reverting back to zero (since some agreements are terminated), our analysis with this data focuses on the years prior to 2012 when most of the terminations occurred.

Using these data, we construct a panel of CZs in the continental United States from 2005 to 2020. Our outcome variable of interest is an indicator for whether any local law enforcement agency within a CZ has an active 287(g) agreement in a given year.[10] While all of the data we consider exist at the county level, we focus on commuting zones. If individuals respond to local labor market conditions, the locality they consider likely expands beyond their county of residence. However, in Appendix Table A2, we show our results are robust to fixing geography at the county.[11]

IV Empirical Strategy

To identify the impact of the Great Recession on the likelihood to adopt a 287(g) program we adopt the methodology in Yagan (2019) which exploits spatial variation in the Great Recession's severity to study its long-term impact on employment and earnings. Our main regression is:

$$Has287g_{zt} = \beta_1 Post_t \cdot Shock_z + \alpha_z + \lambda_t + \epsilon_{zt} \tag{1}$$

where $Has287g_{zt}$ is an indicator for whether any county within a commuting zone, z, has adopted a 287(g) agreement in year t; $Post_t$ is an indicator that equals one in the years after the onset of the Great Recession (2007); $Shock_z$ is a measure of the impact of the Great Recession on a commuting zone, defined as the change in the unemployment rate from 2007 to 2009 ; α_z and λ_t are standard

[10] In alternative specifications, we account for the varying number of jurisdictions across CZs by altering the outcome variable to represent the share of the population covered by a 287(g) program. These specifications are advantageous because they can also address the issue of statewide 287(g) programs. See Appendix Table A3 for results.

[11] We further show the robustness of our results to varying levels of geography in Section VI.

commuting zone and year fixed effects and ϵ_{zt} is our idiosyncratic error term.[12] All standard errors allow for the arbitrary correlation in errors at the commuting zone level.

In an additional specification, we include the term $\sum_{t=2000}^{2020} \phi_t(\mathbb{1}\{year = t\} \cdot X_z^{2000})$, where ϕ_t captures potentially time-varying effects of initial commuting zone-level characteristics. These baseline characteristics include the share of the commuting zone's population in 2000 without a college degree, identify as a veteran, are foreign born, the change in import competition from China from 2000-2007, the routine-share of employment, and the normal trade relations tariff rates in 1990. We also include interaction terms to account for the phasing out of the global Multi-Fiber Arrangement. We discuss the inclusion of control variables in further detail in Section VI.

Our coefficient of interest is β_1, which captures the change in the likelihood of any law enforcement agency within a commuting zone adopting a 287(g) agreement given a one percentage point increase in the unemployment rate from 2007 to 2009. The underlying assumption in this strategy requires that, in expectation, the change in outcomes for the commuting zones with smaller changes in the unemployment rate due to the Great Recession reflect what the commuting zones with larger changes in the unemployment rate would have experienced had the Great Recession not occurred. We visually test the validity of the common trends assumption by presenting a set of event-study results that allow the effect of the Great Recession to vary by years since its occurrence. Specifically, we run the following regression:

$$Has287g_{zt} = \sum_{l=-2, l\neq -1}^{13} \theta_l Shock_z \cdot \mathbb{1}\{t - 2007 = l\} + \pi_z + \gamma_t + \mu_{zt} \tag{2}$$

where l represents the lag or lead of interest, and 2007 is the year of occurrence. Since we omit the year before the Great Recession, each θ_l captures the effect of the shock relative to the year before the Great Recession. Importantly, we must also assume that there are no shocks related to the adoption of immigration enforcement partnerships that coincide with the timing of the Great Recession and are correlated to its severity. We further discuss the validity of our empirical strategy in Section VI.

V The Impacts of the Great Recession on 287(g) Adoption

We begin by describing the estimated effects of the Great Recession on the likelihood of any LLEA within a commuting zone (CZ) having an active 287(g) program in place. The results of our main analysis are reported in Table 3. Each cell represents the coefficient on the $Post_t \cdot Shock_z$ interaction for the separate regressions. Column (1) reports the results across our entire sample period. We find

[12] We also show our results are robust to the use of binary treatment indicators in Appendix Table A4.

that a 1 percentage point increase in the unemployment rate during the 2007-2009 Great Recession was associated with a 1.3 percentage point (or 25%) increase in the likelihood that any LLEA within a CZ adopted the program. These results hold whether we measure participation as having an active agreement between 2005-2020 (Column 1), having an active agreement between 2005-2011 (Column 2), applying regardless of eventual acceptance between 2005-2011 (Column 3), or focus on the percent of the population under 287(g) jurisdiction within a CZ (Column 4).

The result that economic conditions influence the adoption of immigration policy is significant. Standard economic theory predicts that increased competition in the labor market influences policy preference. However, empirical evidence has provided mixed results (Citrin et al., 1997; Mayda, 2006; Card et al., 2012; Malhotra et al., 2013; Hainmueller and Hopkins, 2014; Cotofan et al., 2024). While most of the existing research examines how the economic consequences of immigration shape public attitudes or electoral outcomes, our results demonstrate that broader economic shocks (unrelated to immigration itself) exert a substantial influence on policy adoption.

One might wonder whether the relationship between local labor market conditions and the adoption of immigration enforcement policies expands beyond the evaluation of 287(g) participation. Following the Great Recession, several policies were introduced at the local level that expanded the efforts of Immigration and Customs Enforcement, including the Secure Communities (SC) and E-Verify programs. There are two primary reasons we focus our analysis on 287(g) rather than the alternative immigration policies: first, is program timing, neither Secure Communities nor E-Verify were adopted prior to the Great Recession, and second, 287(g) programs require active engagement from LLEAs. However, prior work evaluating the relationship between negative, local labor market shocks and the adoption of SC and E-Verify found that areas hit harder by the Great Recession were more likely to adopt these policies (Barrera et al., 2025).

One concern is that our estimates are confounded by the Trump administration's expansion of the 287(g) program beginning in 2016, when participation increased from 32 to 152 agencies, and by the removal of institutional guardrails (American Civil Liberties Union, 2025). If this expansion introduced new determinants of program adoption that happen to correlate with recession severity, our full-sample estimates would confound the causal effect of the recession with the effect of Trump era policy interventions, specifically those that occurred in his first administration. However, as shown in Column (2) in Table 3, are results are robust when we limit the sample to focus on the years 2005-2011, limiting the role the first Trump administration could play on our findings.

To examine the timing of the effect of the Great Recession on the likelihood to adopt a 287(g) program, we use our event-study specification. The results of Equation (2) allow us to look at the effect (relative to the year 2006) of changes in the unemployment rate due to the Great Recession

in each year of the sample rather than averaging across the entire post-period. Figure 3 plots the results of Equation (2) for our outcome variable of interest. Year 0 indicates the year of the start of the Great Recession, 2007. This figure shows a relatively large jump in the probability of adopting a 287(g) agreement starting in 2009 that remains statistically significant throughout the remainder of the sample period.

The increase in the coefficient that occurs in 2019 and 2020 may reflect the change in ICE's strategy under the first Trump Administration. Initially, applications were evaluated on whether the LLEA had sufficient resources to expand the duties of the agencies this shifted as ICE sought to maximize the number of 287(g) agreements. Therefore, it is possible that these agencies may have wanted to sign a 287(g) agreement in response to the Great Recession but where otherwise prevented from doing so by ICE. In order to evaluate this claim, we compare the universe of agencies that expressed interest between 2005-2011 with the universe of signed agreements after 2016. Consistent with this hypothesis, we see that more than half of the commuting zones that applied but failed to implement between 2005-2011 eventually had active 287(g) agreements after 2016. Therefore, we interpret the spike in 2019 and 2020 as a reflection of agencies interested in signing agreements after the recession, but having to wait for a more generous policy environment to implement.

V.A Heterogeneity by Baseline Commuting Zone Characteristics

We have found consistent evidence that there is a strong association between the severity of the Great Recession and the likelihood of any LLEA within a commuting zone (CZ) having an active 287(g) agreement. However, these average estimates across all commuting zones facing relatively larger changes in the unemployment rate between 2007 and 2009 could differ across various subgroups. Therefore, we disaggregated the results by the following baseline characteristics: Non-Hispanic, minority share of the population, Hispanic share of the population, foreign-born share of the population, incarceration rate, construction share of the workforce, agriculture share of the workforce, and foreign-born over-representation in routine manual jobs. We calculated these estimates by introducing interaction terms of the CZ subgroup with the $Post_t \cdot Shock_z$ variable in Equation (1).

Table 4 displays the results of our heterogeneity analysis by CZ subgroup for our four measures of 287(g) participation: having an active agreement between 2005-2020, having an active agreement between 2005-2011, applying regardless of eventual acceptance between 2005-2011, and the percent of the population under 287(g) jurisdiction within a CZ, respectively. Panel A shows the differences in outcomes for commuting zone with an above- or below-median foreign-born share of the population. The interaction term in Panel A isolates the impact of the Great Recession on the CZs facing the largest pool of immigrant competition. We find across all of our measures of interest, 287(g)

participation is driven by the set of CZs with larger immigrant shares. These results are consistent with Group Threat theory, which posits that when resources are perceived to be scarce, a large "out group" population (such as the immigrant community) will trigger greater anxiety amongst "in-group" members (in this case, the native community) (Blalock, 1967; Esses et al., 2001; Schlueter and Scheepers, 2010).

Panel B displays the differences in outcomes for commuting zones with above- and below-median incarceration rates in the baseline year (2000). One might expect that CZs with an higher initial incarceration rates may be more likely to adopt a 287(g) program because the policy is marketed towards improving safety in local neighborhoods. However, across all of the columns, the estimate on the interaction terms with above-median incarceration rate is statistically insignificant. This result is significant because it contradicts the claim that the adoption of these programs were driven by differences in pre-Great Recession crime (for which incarcerations are a rough proxy) or intensity of policing behavior.

In Panels C-E, we highlight the role that labor market competition plays in the adoption of 287(g) agreements by examining the differences in outcomes for commuting zones based on initial industry composition. Panel C focuses on the differences for CZs with an above- or below-median share of the workforce in the construction industry. We begin with the construction industry because (1) it was one of the hardest hit industries during the Great Recession, and (2) it is an industry where non-college educated natives and undocumented immigrants compete for occupations that provide a livable wage. Therefore, areas more reliant on the construction industry prior to the Great Recession may be more likely to participate in a 287(g) program if the scarcity of these jobs following 2007 increased the competition between these two groups of workers. We find across all of our outcome variables, 287(g) participation is largest in CZs that had an above-median share of the workforce employed in the construction industry, lending support for this hypothesis.

In Panel D when we examine differential impacts based on the share of the workforce in the agriculture industry. Across all outcome variables of interest, the estimate on the interaction term with above-median share of the workforce in the agriculture industry is negative, statistically significant, and almost equal in magnitude to the overall estimate on the $Post_t \cdot Shock_z$ variable. These findings imply that the changes we see 287(g) participation are driven by the CZs that face relatively larger increases in the unemployment rate from 2007 to 2009 and were originally not reliant on the agriculture industry. In fact, CZs hit harder by the Great Recession with an above-median baseline agriculture share see small or no changes in the outcomes of interest when compared to the control group. These results may be due to the fact that the agriculture industry is one with an outsized undocumented immigrant workforce, and a lack of demand from the native born population to fill

these jobs, suggesting perceived labor market competition following the Great Recession may be muted in areas that were initially reliant on this industry.

Hershbein and Kahn (2018) show that Metropolitan Statistical Areas (MSAs) hit harder by the Great Recession saw the largest decrease in routine manual occupations. This finding can help support our hypothesis that labor market competition drives the adoption of 287(g) adoption if (1) foreign-born individuals are over-represented in these occupations and (2) CZs adopting 287(g) have higher levels of initial over-representation. Panel E presents the differential impacts on 287(g) adoption by the baseline percentage point difference between the share of foreign-born individuals working in routine-manual jobs and the general population. Specifically, the interaction term includes an indicator for whether the CZ has an above-median baseline over-representation of foreign-born workers in routine-manual occupations.[13]

VI Threats to Validity and Robustness

The previous section shows that increases in the unemployment rate driven by the Great Recession is associated with an increased likelihood of a local law enforcement agency having an active a 287(g) program in place. There remain, however, several potential threats to validity that should be addressed. Specifically, (1) our main specification relies on a continuous treatment variable, (2) the impact of the Great Recession on 287(g) adoption may be driven by differential trends across areas with larger versus smaller changes in the unemployment rate from 2007 to 2009, and (3) the results may be sensitive to the geographic level of analysis and measurement of Great Recession shock.

Callaway et al. (2024) shows that when using a continuous treatment to estimate the average treatment effect in a two-way fixed effect design, the identifying assumption is stronger than the one discussed in the previous section. Specifically, the strong parallel trends assumption requires that the path of outcomes for lower-dose units reflects how higher-dose units' outcomes would have changed had they instead experienced the lower dose. We address this issue by leveraging the

[13]We calculate the over-representation of foreign-born individuals in routine manual occupations using the 2005 American Community Survey (ACS). Specifically, we map the public use microdata areas (PUMAs) available in the ACS to CZs to calculate the foreign-born share of the population. Then, following Autor and Dorn (2013), we calculate the foreign-born share of individuals in routine manual jobs by CZ. The difference between the foreign-born share of individuals in routine manual jobs versus the population reveals the over-representation of the group by CZ. We find, across all of our outcome variables of interest, that CZs hit harder by the Great Recession with an above-median foreign-born over-representation in routine-manual occupations witness larger increases in 287(g) participation. We take these results as strong evidence that the increases in immigration enforcement adoption we document for CZs is driven by increases in labor market competition following the Great Recession.

Callaway et al. (2024) estimator and plotting results in an event-study format.[14] Appendix Figure A2 shows the results of this exercise. We continue to find similar results to those in our preferred specification, suggesting our results do not hinge on the stronger assumption made in the standard difference-in-differences model.

To ensure our findings are not driven by differential pre-trends across CZs, we present event-study specifications that examine whether CZs hit harder by the Great Recession saw changes in population and economic characteristics prior to 2007 that might explain the increased likelihood of having an active 287(g) agreement. Specifically, we combine population estimates from the Surveillance, Epidemiology, and End Results (SEER) Program (National Cancer Institute, 2025) and the National Vital Statistics System (National Center for Health Statistics, 2025) with economic estimates from the Bureau of Labor Statistics (U.S. Bureau of Labor Statistics, 2025) to construct a panel of these characteristics from 2000 to 2019.[15] We use this panel to re-estimate Equation 2 where the outcome variables of interest include CZ level measures of population growth, the share of the population aged 20 and above that identify as Hispanic,[16] the birth rates of foreign born mothers, the employment growth rate, the unemployment rate, and the employment-to-population ratio.[17]

Figure 4 presents the results of this exercise. Prior to 2007, both CZs that experienced a relatively larger and smaller increase in the unemployment rate during the Great Recession appear to have similar trends in our population and economic characteristics, as shown by the relatively flat differences between the two groups. In all years before 2007 (except for very small differences in specific years for the population and employment growth rate), the 95 percent confidence interval contains zero, which means that in those years, the difference between areas high and low exposure areas cannot be distinguished from the value in the year before the Great Recession began. We do find some evidence that *following* the Great Recession, CZs hit harder saw significant reductions in the Hispanic share of the population (Panel B) and the birth rate to foreign-born, Hispanic mothers (Panel C). These findings are in-line with prior work showing that location choices of Mexican-born

[14]There are important caveats to the use of this estimation strategy. Specifically, in its current stage, the packages used to implement the strategy require that there exist units that are untreated (receive a dose of zero). There are three commuting zones that witness either no change or a decrease in the unemployment rate. We consider these units as untreated.

[15]There small are differences in years of available data across each of the outcome variables. Specifically, we only have access to data on the birth rates of foreign-born mothers up to 2018 and we can not estimate growth rates for the year 2000.

[16]Ideally we would examine changes in the share of the population that were born outside of the United States, but that information is not available from SEER. In Appendix Figure A3, we show the results are similar when examining the share of the population aged 20 and above that identify as non-White.

[17]The analysis of the economic characteristics matches the estimates reported in Hershbein and Kahn (2018), which evaluated these outcomes at the Metropolitan Statistical Area level.

immigrants was dependent on the severity of the Great Recession (Cadena and Kovak, 2016).

We further address any concern that changes in these variables could impact the likelihood of having an active 287(g) agreement by re-estimating our model when controlling for the baseline differences across CZs. Specifically, we use inverse probability weighting to create comparable treatment and control commuting zones based on pre-2007 levels of population growth, employment growth rate, the unemployment rate, the employment-to-population ratios and the variables described in Section IV.[18] These variables are used to create a propensity score when treatment is defined as having an above median increase in the unemployment rate between 2007 and 2009. The results are presented in Appendix Table A5. We continue to find robust evidence that CZs hit harder by the Great Recession were more likely to have a 287(g) agreement in place.

The third concern is that our results are sensitive to the specific definition of commuting zone we employ and our measurement of the severity of the Great Recession. We begin to address these concerns by re-estimating our baseline estimate (from Column (1) in Table 3) using the definitions of commuting zones from the United States Department of Agriculture Economic Research Service in 1990 and 2000. We find consistent evidence that, regardless of the definition of commuting zone, the sign, magnitude and significance level of the interaction term of interest holds as shown in Appendix Table A6. One may also question whether commuting zones are even the appropriate economic area to consider in our analysis. We examine the robustness of our results to the use of Metropolitan Statistical Areas (MSAs) (rather than CZs) in Columns (1) and (2) of Appendix Table A7. We continue to find strong evidence that areas hit harder by the Great Recession were more likely to have a 287(g) agreement in place. Our results are also not sensitive to our measure of the severity of the Great Recession. In Columns (3) and (4) of Appendix Table A7, we follow Hershbein and Kahn (2018) and rely on MSA-specific changes in projected annual employment growth between 2006 and 2009 with the use of a Bartik shock.[19] Again, we find results in line with our baseline specifications.

VII Alternative Mechanisms to Labor Market Competition

An additional threat to the validity of our identification strategy, not discussed in the previous section, is that the increase in 287(g) adoption following the Great Recession may be driven by factors beyond the economic shock itself. In particular, localities may have adopted 287(g) agreements in response to changes in the political environment, rising criminal activity, or public safety concerns that coincided with the severity of the Great Recession. If areas hardest hit by the Great Reces-

[18] See Roth et al. (2023).

[19] An additional benefit to this specification is that we abstract from any potential impact of 287(g) agreements on local area unemployment between 2007 and 2009.

sion also experienced changes in these outcomes that exogenously motivated 287(g) adoption, our estimates would conflate the causal effect of economic distress with these omitted factors. In the following subsections, we assess the potential role of each of these alternative mechanisms.

VII.A The Political Effects of the Great Recession

Sheriff's departments make up a vast majority of 287(g) agreements during our sample time frame. We highlight this fact as sheriffs are elected at the county level in 46 out of 50 states, suggesting a potential role for the local political environment to influence the policies adopted by these local law enforcement agencies. If areas hit harder by the Great Recession saw shifts in the political environment towards right wing parties that favor enacting stricter immigration policies, or towards more right-wing representatives, the increase in active 287(g) programs may be due to the shifts in the political atmosphere rather than the changes in labor market conditions.

Prior work studying the political effects of economic conditions provides the precedent for this idea. For example, labor market crowding caused by immigration in 1850s Massachusetts increased support for the nativist Know Nothing Party (Alsan et al., 2020), while rising trade exposure shifted voters toward more extreme political candidates (Autor et al., 2020; Choi et al., 2024). In Europe, immigration has been shown to increase support for far-right, anti-immigrant parties, with effects concentrated in areas facing higher unemployment (Halla et al., 2017; Edo et al., 2019). Non-immigration centered economic shocks have also been shown to influence political outcomes. Local economic shocks from rising import competition increased political polarization and extremism in the United States Autor et al. (2020), and support for nationalist and isolationist parties in Western Europe (Colantone and Stanig, 2018; Dippel et al., 2022). Furthermore, work evaluating the Great Recession in Europe shows that the increases in the unemployment rate increased voting for anti-establishment parties, depressed voter turnout, and eroded trust in European institutions (Algan et al., 2017; Dehdari, 2022; Bo' et al., 2023; Guiso et al., 2024)

We begin to examine the political effects of the Great Recession in the United States by evaluating changes in campaign contributions, voting outcomes and congressional member ideology following Autor et al. (2020). Using data from the Database on Ideology, Money in Politics, and Elections (Bonica, 2024) and Dave Leip's Atlas of US Elections (Leip, 2024) between 2000 to 2018, we construct CZ-level measures of the total number of individual campaign contributors to left wing, moderate and right wing candidates, and vote shares to the Republican party in congressional races. We then examine changes in congressional ideologies by constructing county-congressional district measures of the likelihood of electing left wing, moderate and right wing congressional members. We focus on elections surrounding the House of Representatives as we have consistent data throughout our

sample time frame and its members represent local congressional districts within their state.

We then re-run Equation 1 using the new outcome variables of interest and report the findings of this analysis in Table 5. One might expect that if the adoption of 287(g) agreements were driven by changes in the political environment, rather than labor market conditions, CZs facing a larger change in the unemployment rate from 2007 to 2009 would have experienced shifts to the right as anti-immigrant rhetoric rose within the Republican party. However, we find that the unemployment increase from the Great Recession is associated with a decline in Republican vote shares, log contributors, and the probability of electing right wing politicians. In fact, we see an increased preference for politically moderate politicians in areas harder hit by the Great Recession. We take these results as evidence that the change in local immigration policy was not driven by a general preference shift in favor of more right wing policies.

VII.B The Impact of the Great Recession on Crime

Immigration and Customs Enforcement promotes the 287(g) program as a partnership with local law enforcement intended to enhance public safety by identifying and removing criminal aliens who are amenable to removal from the United States (U.S. Immigration and Customs Enforcement, 2024). This stated motivation raises two concerns for our identification strategy. First, if areas more severely affected by the Great Recession exhibited differential trends in criminal activity prior to 2007, our estimates could conflate the effects of the labor market shock with those pre-existing trends. Second, if the Great Recession itself led to an increase in crime rates, it would be difficult to disentangle any impact of the labor shock from contemporaneous changes in criminal behavior that may have lead to increased adoption of the program.[20] Table 4 showed that our results were not driven by CZs with pre-period higher incarcerations rates. However, these results are not sufficient in addressing whether adoption was driven by changes in crime before or after the Great Recession.

We assess whether areas more severely affected by the Great Recession exhibited systematic differences in crime rates by re-estimating the event-study specification in Equation 2 over the period 2000–2020. Using data from the Uniform Crime Reporting (UCR) Program (Federal Bureau of Investigation, 2025) and the SEER Program (National Cancer Institute, 2025), we construct commuting zone–level measures of total, economic, and violent crime rates per 100,000 residents. We find no evidence that areas experiencing larger increases in unemployment between 2007 and 2009 saw greater increases in crime either before or after the onset of the Great Recession relative

[20]There is a large literature on the relationship between labor market conditions and crime (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Lin, 2008). However, the impact of the Great Recession on crime rates remains an open question.

to less affected areas. We interpret the findings in Figure 5 as inconsistent with the notion that the rise in 287(g) program adoptions was driven by changes in local criminal activity.

We further evaluate the role of public safety as a a mechanism for 287(g) policy adoption by examining changes in Notices to Appear (NTA). NTAs are formal documents that initiate removal proceedings and can be broadly categorized into criminal NTAs and non-criminal NTAs.[21] NTAs are issued by ICE or authorized agents (e.g. 287(g) participants) when they are placed into removal proceedings, and include information on the criminal charges of individuals encountered by law enforcement. If 287(g) partnerships were adopted as tools for enhancing public safety we would expect to see a notable rise in criminal NTAs following activation. In contrast, if enforcement was focused more broadly on civil immigration violations, we would observe a rise in non-criminal NTAs instead. Examining whether 287(g) participating agencies experienced differential trends in criminal NTAs prior to program adoption, we can further evaluate whether our results are driven by pre-existing changes in crime rather than the Great Recession itself.

To test this, we estimate an event study centered on the activation year of 287(g) agreements using an event analysis. We track the number of criminal and non-criminal NTAs per 10,000 population across time relative to the year of activation. We situate our analysis at the county-by-year level given the granularity of the NTA data. While the public justification for 287(g) partnerships has consistently emphasized criminal control and public safety, we find that the actual pattern of enforcement diverges sharply from this narrative. The results suggest that non-criminal NTAs increase sharply and persistently following activation, while criminal NTAs show no meaningful change. This divergence is illustrated in Figure 6, revealing that most enforcement activity under 287(g) targeted individuals who violated immigration laws rather than those posing a criminal threat.

VIII Conclusion

In this paper we study how labor market shocks impact local immigration enforcement by examining the relationship between geographic variation in the intensity of unemployment increases from Great Recession and 287(g) agreements.

We find the that commuting zones that experienced larger unemployment shocks from the Great Recession were more likely to sign a 287(g) agreement. We rule out that this relationship between 287(g) agreements and unemployment increases was driven by changes in demographic composition, rightward political polarization, and increases in crime in areas that experienced larger unemploy-

[21] Criminal NTA charges are based on felony charges or other criminal conduct while non-criminal NTA charges are based on civil immigration violations such as entry without inspection (EWI) or other immigration-only charges (OIC).

ment increases. We therefore propose that the increase in local immigration enforcement in areas more impacted by the recession is driven by native anxieties of immigrants competing for scarcer labor market opportunities. Consistent with this hypothesis we find that impacts are stronger in areas where immigrants are more likely to be perceived as competitors. These areas are commuting zones with a higher foreign born share, commuting zones with a higher share of the population working in construction, an industry with a large native and foreign born share of workers, and commuting zones with more foreign over-representation in routine manual occupations, an occupation that experienced large persistent employment losses (Hershbein and Kahn, 2018).

These findings are important because we show that downturns in the business cycle can not only impact local openness to immigrations but also lead to policies that impact a local communities ability to recover from serious economic downturns. Additionally, our findings that immigration enforcement is influenced by local economic conditions, will allow future research to better estimate the impacts of policies like 287(g) by better controlling for local economic conditions. This is of the utmost importance, since 287(g) agreements have become more and more popular, since as of December of 2025 more than 1200 of these agreements with local lawn enforcement agencies have been signed (Immigration and Customs Enforcement, 2025).

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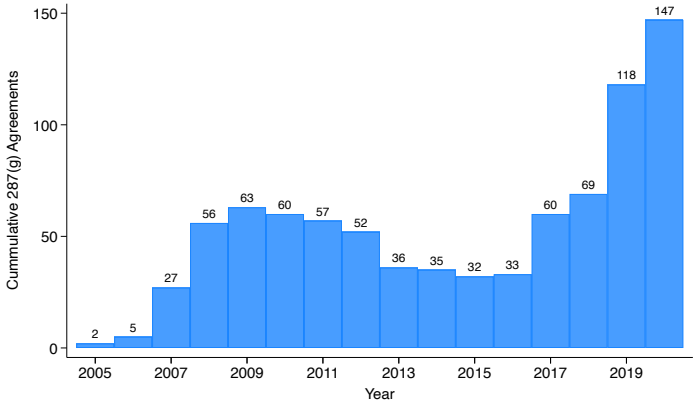
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Figures

Figure 1: Number of Active 287(g) Nationwide

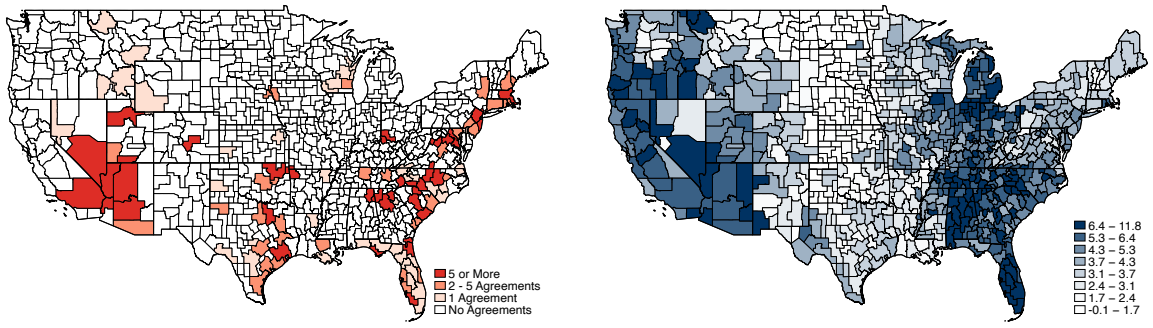


Notes: This figure depicts the number of active 287(g) agreements from local law enforcement agencies across the United States for each year from 2005 to 2020. Data on the number of agreements comes from the U.S. Immigration and Customs Enforcement archives on 287(g) memorandums of agreement/understanding.

Figure 2: Geographic Variation

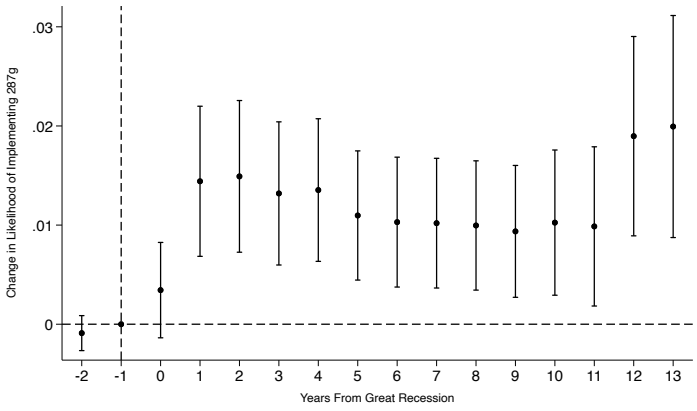
(a) Adoption of 287(g) 2005-2020

(b) Great Recession Shock



Notes: This figure depicts the number of active 287(g) agreements across the United States for each year from 2005 to 2020. Data on the number of agreements comes from the U.S. Immigration and Customs Enforcement archives on 287(g) memorandums of agreement/understanding.

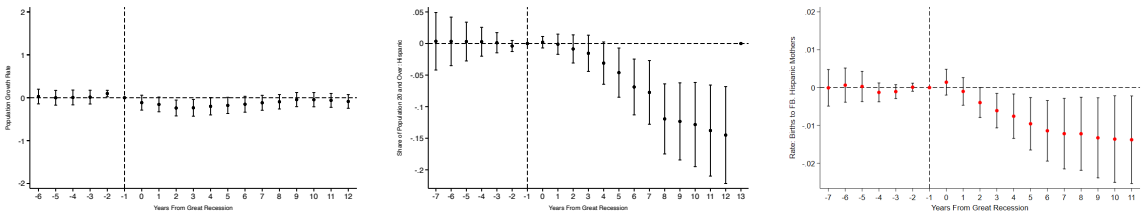
Figure 3: Impacts on 287(g) Adoption



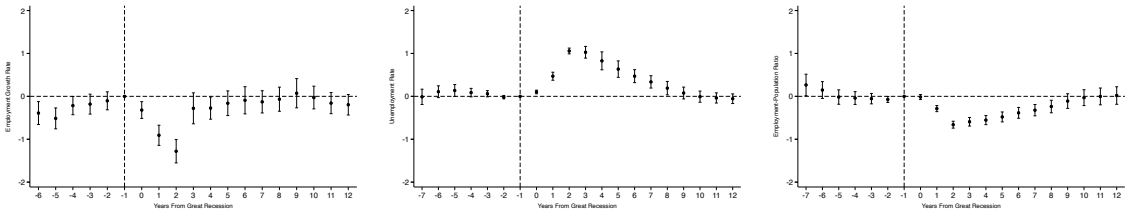
Notes: This figure depicts the event-study estimates from Equation 2 on the likelihood of any local law enforcement agency having an active 287(g) agreement within a commuting zone. 95% confidence intervals are reported. All estimations include commuting zone and year fixed effects. Data on the number of agreements comes from the U.S. Immigration and Customs Enforcement archives on 287(g) memorandums of agreement/understanding.

Figure 4: Trends in Population and Economic Characteristics

(a) Population Growth Rate (b) Share Hispanic (c) Births to For.-Born Mothers



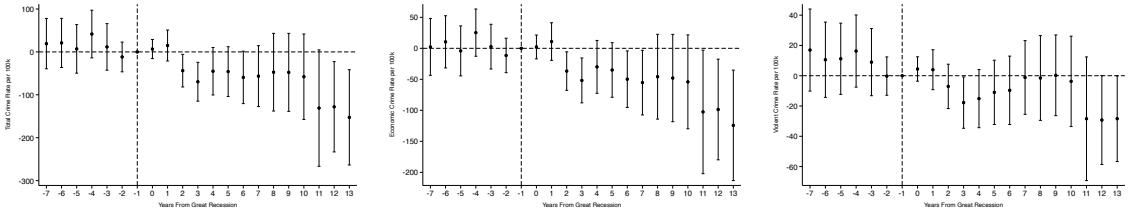
(d) Employment Growth Rate (e) Unemployment Rate (f) Emp.-to-Pop. Ratio



Notes: This figure depicts the event-study estimates from Equation 2. Figure 4(a) plots the results on the population growth rate, Figure 4(b) plots the results for the share of the population that is Hispanic, Figure 4(c) plots the estimates of birth rates to foreign-born mothers, Figure 4(d) plots the estimates for the employment growth rate, Figure 4(e) plots the estimates for the unemployment rate, and Figure 4(f) plots the estimates for the employment-to-population ratio. 95% confidence intervals are reported. All estimations include commuting zone and year fixed effects. Data on population measures comes from the Surveillance, Epidemiology, and End Results Program. Data on birth rates come from the National Vital Statistics System. Data on economic characteristics come from the Bureau of Labor Statistics.

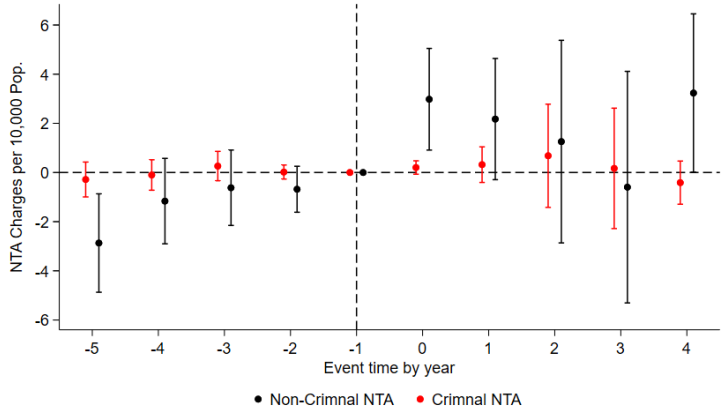
Figure 5: Crime Trends

(a) Total Crime Rate per 100k (b) Economic Crime Rate per 100k (c) Violent Crime Rate per 100k



Notes: This figure depicts the event-study estimates from Equation 2. Figure 5(a) plots the results for the total crime rate per 100k, Figure 5(b) plots the results for the economic crime rate per 100k, and Figure 5(c) plots the results for the violent crime rate per 100k. 95% confidence intervals are reported. All estimations include commuting zone and year fixed effects. Data on crime levels is from the Uniform Crime Reporting System and population data are from the Surveillance, Epidemiology, and End Results Program.

Figure 6: Notice to Appear Event Analysis



Notes: This figure presents an event study of criminal and non-criminal NTAs per 10,000 population in the years before and after 287(g) activation. Event time is centered on the year prior to 287(g) adoption (t=-1). Criminal NTAs are based on felony charges or other criminal violations, while non-criminal NTAs are based on civil immigration violations such as entry without inspection or other immigration-only charges.

Tables

Table 1: Descriptive Statistics of Active 287(g) Programs from 2002-2020

Year	# of Unique Programs In Place	# of Terminated Programs	Share of Programs from State Agency	Share of Programs from Sheriff's Departments	Share of Programs from Other Agencies	Share of Programs - Jail Model	Share of Programs - Task Force Model	Share of Programs - Joint Model	Share of Programs - WSO Model
2002	1	0	100	0	0	0	100	0	0
2003	2	0	100	0	0	0	100	0	0
2004	2	0	100	0	0	0	100	0	0
2005	5	0	60	40	0	60	40	0	0
2006	8	0	37.5	62.5	0	75	25	0	0
2007	35	0	22.85	57.15	20	48.57	28.57	22.86	0
2008	67	0	16.42	61.19	22.39	46.27	31.34	22.39	0
2009	76	6	17.11	59.21	23.68	47.37	32.89	19.74	0
2010	73	2	17.81	58.9	23.29	46.58	31.51	21.92	0
2011	69	1	17.39	59.42	23.19	47.83	28.99	23.19	0
2012	59	23	11.86	61.02	27.12	52.54	28.81	18.64	0
2013	38	0	5.26	81.58	13.16	100	0	0	0
2014	37	2	5.41	81.08	13.51	100	0	0	0
2015	34	2	5.88	79.41	14.71	100	0	0	0
2016	35	0	5.71	80	14.29	100	0	0	0
2017	62	2	3.23	87.1	9.68	98.39	0	0	1.61
2018	72	2	4.17	87.5	8.33	98.61	0	0	1.39
2019	121	7	2.48	93.39	4.13	61.98	0	0	38.02
2020	152	2	3.29	92.76	3.95	49.34	0	0	50.66

Notes: This table reports descriptive statistics on active 287(g) agreements across the sample period. Information on 287(g) agreements come from archived records of memorandums of understanding available on the U.S. Immigration and Customs Enforcement website Immigration and Customs Enforcement (2025). Other agencies refer to city/county police departments and county jail/corrections departments. WSO stands for Warrant Service Officer.

Table 2: Summary Statistics of CZs Above and Below Great Recession Shock

	Above Median	Below Median	**Difference**
	(1)	(2)	(3)
Total Population	527,926.20	240,301.10	-287,625.10***
	(1,341,976.00)	(612,220.70)	
Median Household Income (in '000s)	34.84	32.65	-2.20***
	(7.18)	(5.99)	
% Pop. Over 25 - No College	36.52	35.02	-1.50**
	(6.96)	(6.19)	
% Pop. Over 25 - Bachelor's Degree	7.37	8.05	0.68***
	(2.75)	(2.48)	
% Pop. Non-Hispanic, White	78.48	81.84	3.36*
	(17.99)	(17.67)	
% Pop. Hispanic	6.55	8.68	2.13*
	(12.72)	(14.32)	
% Pop. Foreign Born	4.27	3.71	-0.55
	(5.37)	(4.20)	
% Pop. Working in Construction Industry	3.36	3.12	-0.25***
	(1.06)	(0.84)	
% Pop. Working in Agriculture Industry	1.81	4.50	2.69***
	(1.62)	(3.83)	
Δ in Share Employment in Offshorable Jobs 1990-2000	-0.53	-0.70	-0.17***
	(0.42)	(0.41)	
Δ in Share Employment in Manufacturing 1990-2000	22.34	15.70	-6.64***
	(11.22)	(9.86)	
Δ in Share Employment in Routine Jobs 1990-2000	29.78	27.84	-1.94***
	(2.74)	(2.68)	
N	361	361	722

Notes: This table presents summary statistics for commuting zones that experienced an above median (column 1) increase in the unemployment rate between 2007 and 2009 versus a below median change (column 2) in the year 2000. Data on demographic characteristics come from the U.S. Census Bureau, while economics characteristics come from Autor et al. (2021).

Table 3: Impact of Great Recession on 287(g) Participation

VARIABLES	(1) Has 287(g) 2005-2020	(2) Has 287(g) 2005-2011	(3) Applied 287(g) 2005-2011	(4) %Pop Covered 287g 2005-2020
$Post_t \cdot Shock_z$	0.013***	0.010***	0.017***	0.57***
	(0.003)	(0.004)	(0.0074)	(0.168)
Post-Period Mean	0.053	0.029	0.061	2.35
Observations	11,552	9,386	9,386	11,552

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. $Shock_z$ is defined as the change in the unemployment within a commuting zone from 2007 to 2009 as in Yagan (2019).

Table 4: Heterogeneity Results by Commuting Zone Characteristics

VARIABLES	(1) Has 287(g) 2005-2020	(2) Has 287(g) 2005-2011	(3) Applied 287(g) 2005-2011	(4) %Pop. Covered by 287g 2005-2020
Panel A: High Foreign-Born Population Share				
$Post_t \cdot Shock_z$	0.002	0.002	0.003	0.090
	(0.002)	(0.002)	(0.003)	(0.112)
Interaction with Above Median	0.023***	0.022***	0.023***	1.017***
Baseline Foreign-Born Share	(0.003)	(0.004)	(0.004)	(0.171)
Panel B: Incarceration Rate (per 100k)				
$Post_t \cdot Shock_z$	0.011***	0.012***	0.014***	0.443**
	(0.004)	(0.004)	(0.004)	(0.190)
Interaction with Above Median	0.002	0.001	0.001	0.216
Baseline Incarceration Rate	(0.004)	(0.004)	(0.004)	(0.177)
Panel C: Share Workforce in Construction				
$Post_t \cdot Shock_z$	0.002	0.002	0.004*	0.035
	(0.002)	(0.002)	(0.002)	(0.110)
Interaction with Above Median	0.020***	0.019***	0.018***	0.985***
Baseline Construction Share	(0.003)	(0.003)	(0.004)	(0.162)
Panel D: Share Workforce in Agriculture				
$Post_t \cdot Shock_z$	0.014***	0.014***	0.016***	0.643***
	(0.003)	(0.003)	(0.004)	(0.173)
Interaction with Above Median	-0.018***	-0.016***	-0.020***	-0.721***
Baseline Agriculture Share	(0.003)	(0.003)	(0.003)	(0.137)
Panel E: Foreign-Born Over-Representation in Routine-Manual Jobs				
$Post_t \cdot Shock_z$	0.001	0.001	0.005	0.016
	(0.003)	(0.003)	(0.003)	(0.135)
Interaction with Above Median	0.016***	0.015***	0.013***	0.795***
Baseline Foreign-Born Over-representation	(0.003)	(0.003)	(0.004)	(0.153)
Observations	11,552	5,054	5,054	11,552

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. $Shock_z$ is defined as the change in the unemployment within a commuting zone from 2007 to 2009 as in Yagan (2019).

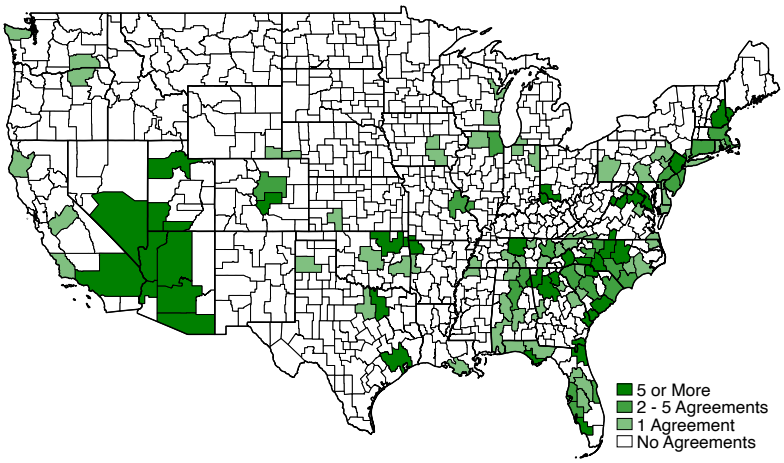
Table 5: The Impacts of the Great Recession on Political Outcomes

Panel A: Total Contributors, Total Votes, and Republican Vote Share			
	Log(# of Contributors)	Log(#of Votes Casted)	Republican Vote Share
$Post_t \cdot Shock_z$	-0.033***	0.032***	-0.395*
	(0.011)	(0.003)	(0.216)
Post-Period Mean	4.454	5.343	61.72
Observations	6,880	7,923	7,923
Panel B: Contributors by Ideology			
	Log(# Left Wing Contributors)	Log(# Moderate Contributors)	Log(# Right Wing Contributors)
$Post_t \cdot Shock_z$	0.008	-0.014	-0.012
	(0.013)	(0.012)	(0.013)
Post-Period Mean	3.300	2.894	3.825
Observations	6,232	5,980	6,694
Panel C: Congressional Member Ideology			
	Elects Left-Wing Candidate	Elects Moderate Candidate	Elects Right-Wing Candidate
$Post_t \cdot Shock_z$	-0.001	0.023***	-0.022***
	(0.002)	(0.006)	(0.006)
Post Period Mean	0.141	0.267	0.592
Observations	37,493	37,493	37,493

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. $Shock_z$ is defined as the change in the unemployment within a commuting zone from 2007 to 2009 as in Yagan (2019).

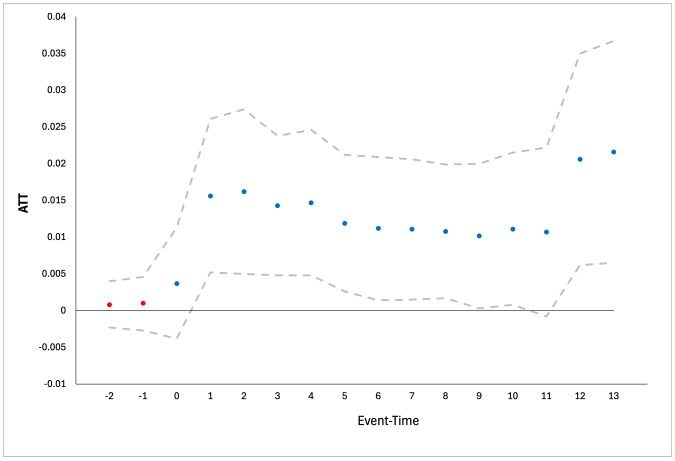
A1 Appendix Figures and Tables

Figure A1: Geographic Variation in Application for 287(g) Programs



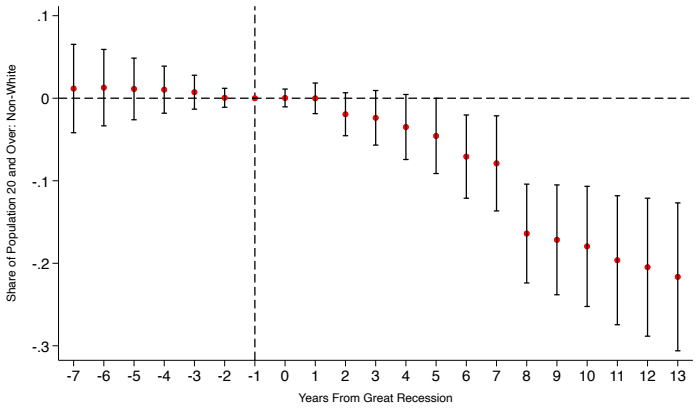
Notes: This figure depicts the number of 287(g) applications across the United States for each year from 2005 to 2011. Data on the number of agreements comes from the U.S. Immigration and Customs Enforcement archives on 287(g) memorandums of agreement/understanding. Information on applications for the 287(g) programs comes from Pedroza (2019).

Figure A2: Event-Study Results Using CBGS(2025)



Notes: This figure depicts the event-study estimates from Callaway et al. (2024) on the likelihood of any local law enforcement agency having an active 287(g) agreement within a commuting zone. 95% confidence intervals are reported. All estimations include commuting zone and year fixed effects. Data on the number of agreements comes from the U.S. Immigration and Customs Enforcement archives on 287(g) memorandums of agreement/understanding.

Figure A3: Event-Study Results for Share Population - Non-White



Notes: This figure depicts the event-study estimates from Equation 2. The figure plots the results for the share of the population that is non-White. 95% confidence intervals are reported. All estimations include commuting zone and year fixed effects. Data on population measures comes from the Surveillance, Epidemiology, and End Results Program.

Table A1: Descriptive Statistics of Denied 287(g) Programs from 2005-2011

Year	# of Unique Unsuccessful Applications	Share Applications from Sheriff's Departments	Share Applications from Other Agencies	Share Denied	Share Withdrawn	Share Unknown
2005	4	25.00	75.00	25.00	75.00	0.00
2006	17	58.82	41.18	47.06	52.94	0.00
2007	66	60.61	39.39	59.09	37.88	3.03
2008	40	45.00	55.00	47.50	45.00	7.50
2009	5	60.00	40.00	40.00	40.00	20.00
2010	6	66.67	33.33	33.33	16.67	50.00
2011	7	100.00	0.00	0.00	0.00	100.00

Notes: This table reports descriptive statistics on unsuccessful 287(g) applications across the years 2005-2011. Information on unsuccessful 287(g) applications come from data made available from Pedroza (2019). Other agencies refer to city/county police departments and county jail/corrections departments.

Table A2: County-level DiD Results

	(1)	(2)	(3)
VARIABLES	Has 287(g) 2005-2020	Has 287(g) 2005-2011	Applied 287(g) 2005-2011
$Post_t \cdot Shock_z$	0.0031***	0.0025***	0.0033***
	(0.0008)	(0.0008)	(0.0009)
Post-Period Mean	0.018	0.014	0.021
Observations	59,109	21,777	21,777

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. $Shock_z$ is defined as the change in the unemployment within a commuting zone from 2007 to 2009 as in Yagan (2019). Post-period mean reports the share of commuting zones with a 287(g) agreement after 2006. Data on applications for 287(g) programs come from Pedroza (2019). Columns (2)-(3) only include the years 2005-2011.

Table A3: DiD Results - Share of 2000 Population Covered by 287(g)

VARIABLES	Share Pop. Covered by 287(g)	Share Pop. Covered by 287(g)	Share Pop. Covered by 287(g)
	(1)	(2)	(3)
$Post_t \cdot Shock_z$	0.577***	0.270	0.730
	(0.170)	(0.496)	(0.550)
Post-Period Mean	2.35	11.41	11.59
Observations	11,552	13,718	12,369
Adjustments	County/City	County/City/State	County/City/State/Removing Banned

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. $Shock_z$ is defined as the change in the unemployment within a commuting zone from 2007 to 2009 as in Yagan (2019). In Column (1), the population considered under a 287(g) agreement only considers local law enforcement agencies. In Column (2), counties in a state with a statewide 287(g) program are also included. In Column (3), we remove states that report banning 287(g) agreements during our sample period.

Table A4: DiD Results Using Binary Treatment Indicator

	Above the Median of $Shock_z$	Top Quartile of $Shock_z$	Top Decile of $Shock_z$
VARIABLES	Has 287g	Has 287g	Has 287g
	(1)	(2)	(3)
$Post_t \cdot Treated_z$	0.039***	0.053***	0.049*
	(0.013)	(0.018)	(0.029)
Post-Period Mean	0.053	0.053	0.053
Observations	11,552	8,672	6,944

Notes: Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. $Treated_z$ is an indicator variable that equals one if a commuting zone is the top 50% (column 1), top quartile (column 2), or top decile (column 3) of the distribution of the change in unemployment rate from 2007 to 2009 as in Yagan (2019). The control group in each specification only includes commuting zones below the median as to not contaminate the new estimates with the treatment effects from those previously considered treated. Post-period mean reports the share of commuting zones with a 287(g) agreement after 2006.

Table A5: DiD Results with Controls

	Baseline	Controls Using Time Trend	Controls with IPW
VARIABLES	Has 287g	Has 287g	Has 287g
	(1)	(2)	(3)
$Post_t \cdot Shock_z$	0.013***	0.008**	0.011***
	(0.003)	(0.003)	(0.004)
Post-Period Mean	0.053	0.053	0.053
Observations	11,552	11,552	11,552
Demographic Controls	No	Yes	Yes
Other Economic Shock Controls	No	Yes	Yes
SEER and BLS controls	No	Yes	Yes

Notes: Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. Additional table notes to be added.

Table A6: Robustness to Various Commuting Zone Definitions

VARIABLES	Has 287(g)	Has 287(g)	Has 287(g)
	(1)	(2)	(3)
$Post_t \cdot Shock_z$	0.0125***	0.0129***	0.0132***
	(0.003)	(0.003)	(0.003)
Post-Period Mean	0.0526	0.0526	0.0564
Observations	11,552	11,552	11,056
Commuting Zone Definition	Autor and Dorn (2013)	USDA ERS 1990	USDA ERS 2000

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects and baseline covariates. $Shock_z$ is defined as the change in the unemployment within a commuting zone from 2007 to 2009 as in Yagan (2019). Post-period mean reports the share of commuting zones with a 287(g) agreement after 2006.

Table A7: DiD Results Using Bartik Shock at MSA Level

	Yagan (2019) Shock		Bartik Shock	
VARIABLES	Has 287(g)	Applied for 287(g)	Has 287(g)	Applied for 287(g)
	(1)	(2)	(3)	(4)
$Post_t \cdot Shock_z$	0.057**	0.068**	0.058**	0.061**
	(0.027)	(0.030)	(0.024)	(0.029)
Post-Period Mean	0.095	0.122	0.095	0.122
Observations	7,201	2,653	7,201	2,653

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All standard errors are clustered at the commuting zone level. Each coefficient is the result of a separate estimation. All regressions include commuting zone and year fixed effects. $Shock_z$ is defined as the change in the unemployment within a commuting zone from 2007 to 2009 as in Yagan (2019).